Vol. 20, No. 4 *PSYCHOLOGICAL REVIEW PUBLICATIONS* April, 1923

Psychological Bulletin

CONTENTS

PUBLISHED MONTHLY BY THE

PSYCHOLOGICAL REVIEW COMPANY

27-29 COLUMBIA STREET, ALBANY, N. Y.

AND PRINCETON, N. J.

AGENTS: G. E. STECHERT & CO., LONDON (2 Star Yard, Carey St., W.C.); PARIS (16, rue de Condé)

Entered as second-class matter at the post-office at Albany, N. Y., September 25, 1922

Vol. 20, No. 4 April, 1923

THE PSYCHOLOGICAL BULLETIN

REACTIONS TO MUSICAL STIMULI

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The experimental literature relating to the psychology of music is already of great extent and could not be readily summarized in a single review. That portion, however, which deals with physiological and overt responses—in short, with behavior in the presence of musical stimuli—is much more limited, and the present review is confined to studies of this nature. Since such studies are relatively few an attempt is made to include all the more important contributions from the very beginnings of research in this field. Such a complete historical survey will be of service as a guide to further experiments in musical esthetics, in which it seems probable that the behavioristic attitude will increasingly predominate. Since many of the experiments deal with reactions to relatively simple musical elements—isolated tones, intervals, chords, etc.—it seems preferable to speak of reactions to musical stimulu, rather than of response to music. With a few exceptions the various papers are discussed in the chronological order of their appearance.

The first observations on the physiological effects of music, which partake of the nature of experiment in the generally accepted sense, seem to be due to the French musician, A. E. M. Gretry (1741-1813) who in his *Essais sur la musique* mentions the effect of music on the heart and the circulation of the blood. "I placed," said he, "three fingers of my right hand on the artery of my left arm, or on any

other artery in my whole body, and sang to myself an air, the tempo of which was in accordance with the action of my pulse; some little time afterward, I sang with great ardor an air in a different tempo, when I distinctly felt my pulse quickening or slackening its action to accommodate itself by degrees to the tempo of the new air" (8). A number of subsequent observers report a repetition of this crude experiment with similar results.

The earliest well controlled experiments on response to auditory stimuli were made in 1874 by Couty and Charpentier (9) as a part of a general study of cardiovascular reactions determined by stimulation of the various senses. The auditory stimuli employed were in part mere noises, but since whistling was also used, the results have some bearing on response to musical elements. The experiments were carried out on dogs, usually under the influence of chloral, strychnine, or curare, and kymographic records were taken of the cardiovascular effects. Twelve experiments in all were made on the effects of sounds on the two dogs employed as subjects. Sudden stimuli, such as whistling, closing the door violently, striking the stove with a shovel, or even accidental noises, generally produced increased cardiac tension and acceleration of pulse ranging from ten to one hundred per cent.

The principal fact brought out by the experiments was that of the diversity of cardiovascular reactions in response to auditory stimuli. The diversity was exhibited not only in different individuals but in the same animal. On the whole, however, the experiments showed that the amplitude of the reactions correspond to the intensity of auditory stimuli, and that if the excitations are continued, adaptation results; the reactions diminishing or disappearing.

To Dogiel, (13) however, belongs the credit of having carried out the first systematic experiments on the influence of music proper on the organism. In 1880 Dogiel executed two series of experiments on the influence of music on the circulation of the blood; the first on human circulation, the second on that of animals, chiefly dogs and rabbits. The experiments on human beings were made by means of the plethysmograph; on animals by taking the cardiac pulse on the carotid artery. The sources of auditory stimuli were König's diapason with resonance boxes, the violin, clarinet, flute, and a metal whistle. The following experiments on animals are representative:

No. of Observation	No. Pulsations in 10 Seconds: Before Auditory Stimulation	During	Immediately After	Blood Pressure mm.: Before Auditory Stimulation	During	After	Condition of Animal
1. Experiments on a small dog, stimulated by menas of a whistle.							
1	20	23	22	130	204	128	Animal was quiet without drug.
2	35	40	37	178	222	210	
3	32	39	204	204	214	214	Curare. Artificial respiration with a little strychnine.
2. Experiment on a small dog (same) with whistle							
1	21	25	..	142	146		Quiet without curare; movements during whistling.
2	24	19	..	148	156		
3	14	16	17	131	133		Curarized. Artificial respiration.
4	15	17	..	128	129		
5	17	19	..	127	130		
3. Experiment on a white rabbit with whistle stimulation.							
1	46	50	50	114		150	Perfectly quiet without curare.
2	48	49	49	112		146	
3	36	40	40	132		140	Curarized with artificial respiration.

Thus the pulse rate increases under the influence of auditory stimuli, especially when the animal is under the influence of strychnine. In cases where pulse rate did not increase markedly, there was an increase of energy. The cardiac rhythm did not remain regular. From the other observations on dogs, cats, and rabbits, the author concludes that auditory stimulation is accompanied by oscillations in the circulation of the blood, and that the latter are dependent on the pitch, intensity, and timbre, of the sound, a result in accord with the observations of Couty and Charpentier.

In the experiments on human beings, diapasons of varying pitch, Sol 3, Sol 4, Mi 4, etc., and a serenade of Schubert's in Mi 3 on the violin, clarinet, and flute were employed as auditory stimuli. The

general conclusions of Dogiel's study are as follows: (1) Music exhibits an influence on the circulation of the blood in man as in animals. (2) Blood pressure sometimes rises and sometimes falls. These oscillations of pressure depend chiefly on the influence of the auditory stimulus on the medulla oblongata which is in relation with the auditory nerve. (3) The action of the musical sounds and of the whistles on animals and man expresses itself for the most part by the acceleration of the cardiac contractions. The automatic centers of the heart work more energetically. (4) The variations of the circulation as a result of musical influence agree with the respiratory changes, although they can be observed independently of the latter. (5) The variations of the circulation depend on the pitch, intensity, and timbre of the sound. (6) In the variations of the blood pressure, the idiosyncrasies of the individual, whether man or animal, are plainly apparent and even the nationality in case of man has some effect.

No further work bearing on the subject appeared until 1885 when Feré and Londe (**15**) investigated the effects of tones produced by vibrating tuning forks on the dynamometric records of a single subject. In the first series of experiments a large number of forks ranging from 50 to 1,000 vibrations per second were employed and large differences in dynamometric pressure, varying with the pitch of the tones, were noted. In another series of experiments, electrically driven forks were used, and the muscular force increased with the intensity of the stimuli. With a tuning fork of 500 vibrations per second, the following results were obtained:

Distance from subject of tuning fork in meters.	Pressure in Kg. on Dynamometer.
	22
8	22
7	24
6	29
5	32
4	35
3	45
2	48
1	52
0	

Approximately the same figures were obtained by placing the subject alternately in different positions in a different order. Feré says that auditory stimuli increase not only the intensity of sudden effort, but also the duration of sustained effort and the power of renewing it.

By means of a weighted tuning fork, yielding the range of the octave, the relative influence of different tones of the scale was ascertained. Two series of experiments were carried out, one involving ordinary transmission, through the air, and the other bone conduction of sound through direct contact with the skull. The results follow:

Through the Air		Through the Skull	
Stimulus	Dynamometer Kg.	Stimulus	Dynamometer Kg.
Ut 2 do	26		50
Re 2	27		45
Mi 2	28		43
Fa 2	28		42
Sol 2	31		35
La 2	35		30
Si 2	38		28
Ut 2 do	45		25

The subject's normal dynamometric record was from 20 to 22 Kg. The dynamometric results agreed with the plethysmographic records under the influence of auditory stimuli. The tone producing maximum reaction varies from subject to subject, the range being the interval of a fifth. The general conclusion is that sounds are dynamogenic, *i.e.*, muscular energy increases with the intensity and pitch of auditory stimuli, a result to be expected in the light of the previous experiments.

In 1887 Lombard (**26**) observed the reinforcement of the knee jerk caused by music, while making a general experimental investigation of the variations of the knee jerk in their relation to the activity of the central nervous system. The apparatus consisted of a hammer suspended on its axis through the handle, falling like a pendulum so that the force of the blow could be regulated, and a rod connected with the foot, sliding in the groove of a wheel, and equipped with a needle which recorded the extent of movement on a blackened surface. His observations may be summarized in the following tables:

Conditions	Knee Jerks	Average
Silence	25 experiments	32 mm.
Music of passing band—air not recorded	60, 71, 74, 70, 60, 55	65 mm.
Another passing band playing "Maryland, My Maryland."	62, 76, 76, 74, 71, 66, 59, 64, 59	67.4 mm.
Drum corps	48, 55, 51, 55, 53, 49, 52	51.8 mm.

There was an increase and a decline in the extent of the knee jerks as music approached and receded. In a subsequent experiment to test the effect of music on the knee jerk an air was played on a piano in an adjoining room. The average knee jerk at 7 P. M. was 32 mm.; at 11 P. M., 29 mm.; so that at 10:30, at the time of the experiment, the "normal" knee jerk probably would have been about 30 mm.

Stimulus	Knee Jerks	Average
Beethoven's Funeral March	82, 104, 96, 105, 104, 99, 108, 95, 106, 118, 117, 90, 113, 119 98, 100, 124, 108, 112	105 mm.
Interval of quiet	83, 90, 90, 56, 82, 59, 75, 50	74 mm.
Chopin's Raindrop Prelude		
Soft music	52, 63, 47, 50, 55	53.4 mm.
Deeper passage	66, 73, 58, 78, 70, 86, 77, 87	74.3 mm.
Soft	66, 43, 59, 62, 71, 68, 54	60.4 mm.
More thrilling	79, 86, 86	83.6 mm.
Varied and softer	58, 57, 79, 67, 47	61.6 mm.
Quiet 15 minutes later	25 experiments	29 mm.

The amplitude of the knee jerk, therefore, seems to vary directly with the intensity of the auditory stimulus.

During the same year, Urbantschitsch (**41**) investigated the influence of auditory stimuli on the sense impressions, particularly on normally minimal visual stimuli. He found that patches of color observed at a distance at which the color could scarcely be recognized were brought clearly into the sensory field, by sounding a tuning fork. In short, the threshold of color perception is lowered by tonal stimuli. Tuning forks of high pitch, applied to both ears, were most effective. The influence of the different colors is variable. Barely legible print was often read when a tone accompanied the effort. Tastes, odors, and touch are similarly affected by sound, and pain is increased by jarring noises. Sound perception is likewise affected by colors and brightness, the ticking of a watch, for example, becoming more distinct under the influence of red and green, less distinct through blue and yellow. The influence of visual perception on musical tones varies with the pitch. Odors also exhibit a marked reinforcing power over sound. Finally Urbantschitsch claims to have produced photisms, or phenomena of colored audition, by having the subject observe a grey disk on white paper, or an undulating

white surface, and describe the colors perceived while different tuning forks were sounded. The experiments, if valid, indicate that the subjects of audition colorée are merely striking examples of the normal psychological influence of one sense upon another.

Little further experimental investigation of reactions to music appeared until the end of the next decade. Eugenio Tanzi,(**38**) however, published an account of experiments on the reaction time for major and minor chords. The keys of a pianoforte, the reagent's key and a chronoscope were connected in a single circuit. The results are given in two tables, one for the simple reaction time, the other for the choice between reaction and no reaction. The conclusion is that minor chords produce more rapid reaction than do the major chords, a fact that may have some relation to the prevalence of minor chords in primitive music.

Th. Billroth (3) in a long theoretical paper on musical reactions presents some very interesting notes on the influence of music on the organism. Most of the paper is devoted to the physiological and motor affects of rhythm, but several observations on tonal effects on man and animals are recorded. The author reports an experiment of his own in which a violent pain appeared in an apparently sound tooth upon hearing a soprano sing a high note (B) at a concert, a quarter tone too high. Examination of the tooth disclosed slight decay. The hyperirritability of the nerve was thus excited by irradiation of an auditory stimulus. The observation accords with the conclusion of Urbantschitsch that auditory stimuli lower the sensory threshold. Billroth also mentions seeing a young "great Dane" fall down as if in a faint when a village brass band struck up a *Schutzenmarsch* ("Es trat eine momentane Lahmungein; er fiel mit vorwärts und ruckwärts gestreckten Beinen hin").

G. Sergi,(**35**) in his work on pain and pleasure (*Dolore e Piacere*, 1894), gives a critical résumé of the experiments of Dogiel on reactions to music. Sergi interprets these results in terms of the James-Lange theory of emotion and concludes that musical emotions entirely lack intellectual character. A paragraph on the dynamometric influence of music appears in Scripture's *Thinking, Feeling and Doing*,(**34**) published during the same year that Tarchanoff's researches appeared. "With the thumb-and-finger grip," says Scripture, "the greatest pressure I can exert during silence is 4 kg. When some one plays the Giants Motive from *Rheingold* my grip shows 4.5 kg. The Slumber Motive from the *Walküre* reduced the power to 3.24 kg."

Among those who have contributed most to our knowledge of reactions to musical stimuli is the Russian physiologist Professor Tarchanoff (**39**). As early as 1888 Tarchanoff noted variations in the electric conductivity of subjects subjected to the stimulus of an electric bell. In later experiments he reported that decapitated animals respond to sound stimuli by violent struggling, although Professor Henri (**21**) was unable to confirm these results, in a similar experiment of his own. In an interesting communication to the International Congress of Rome in 1895, Professor Tarchanoff presented results obtained by means of the Mosso ergograph, which resemble those of Feré's experiments. He notes that music exercises a powerful influence on the muscular activity of man, increasing or diminishing it according to the nature of the melodic stimuli. When a subject is completely fatigued by working on the ergograph and can no longer raise the weight, gay music of rapid movement causes the fatigue to vanish for a variable time, and the subjects are able to raise the weight once more; *i.e.,* do additional work. Sad, slow music in the minor key produces the opposite effect.

A second series of experiments was made on dogs and guinea pigs in order to determine the quantity of oxygen consumed and the quantity of carbon dioxide gas eliminated in 24 hours under varying conditions of rest and auditory stimulation by means of an electric bell sounding every five seconds. During these 24 hours of persistent auditory stimulation the consumption of oxygen increased 12.01 per cent for dogs and 10.94 per cent for guinea pigs. During the same period the elimination of carbonic acid gas increased 11.64 per cent and 11.11 per cent for the respective animals. A third series of experiments on human beings showed that the electric currents determined by the skin of the hand and registered by Wiedemann's galvanometer are modified by the influence of music. Since these cutaneous currents were then regarded as due to increase of secretory activity, Tarchanoff concluded that music influences the activity of the cutaneous glands.

Following the work of Tarchanoff, Dr. Uberto Dutto (**14**), of the laboratory of physiology of Rome, executed a series of tests to determine the influence of musical stimuli on thermogenesis of animals. For the purpose of extending the investigations of Dogiel and Tarchanoff, various animals were subjected to the influence of music by letting them hear an organ during an hour or an hour and one-half. Under these conditions he found that thermogenesis increased in birds generally; hares, guinea pigs, and chickens, however, showed

a diminution. The increase must be attributed to chemical changes in the tissues. Dutto thinks that music determines a state of special psychic tension during which the afflux of blood to the peripheral circulatory system is diminished with a consequent decrease in the radiated heat. This result agrees with the experiments of Mosso, according to which every psychic process determines a peripheral circulatory constriction. If music, as Dutto contends, acts as a stimulus to organic metabolism, we have an explanation of the result of Tarchanoff who found that dogs and guinea pigs consume more oxygen and eliminate more carbon dioxide when subjected to the influence of music.

P. Mentz (**27**) conducted experiments on the influence of auditory stimuli on the movements of circulation and respiration under the varying conditions of attention. The apparatus used consisted of the Marey sphygmograph and pneumograph. He found that auditory stimuli, noises as well as simple sounds, produce a retardation of the pulse, and a retardation or acceleration of the respiration, correlated with the duration of the stimulus and the presence or absence of voluntary attention. The intensity of the auditory sensation increases the duration of reactions up to a certain limit beyond which reactions diminish. The influence of a musical selection played on a harmonium differs according to the attitude of the listener. If he does not pay particular attention or does not attempt to analyze the selection a retardation of the pulse ensues; if he analyzes the music acceleration appears. In an examination of the pulse during the musical selection, Mentz found that a marked variation in intensity produced retardation. Disagreeable dissonances are accompanied by acceleration; agreeable consonance by retardation.

An excellent critical summary of the method and results of these experiments was published during the following year by Prof. Victor Henri (**22**).

The next important series of experiments on reactions to music are those of A. Binet and J. Courtier (**4**). In connection with a study of the capillary circulation of the hand, carried out on four subjects by means of the Marey sphygmograph and the plethysmograph, these authors found a diminution of the amplitude of the pulse in response to the sudden sound of a gong.

A subsequent study by the same investigators (**5**) was devoted to the determination of the energy and rapidity of movements made by a pianist; *i.e.*, the mechanical work of the fingers on the keys.

The study bears most directly upon the influence of technical difficulties in performance, but the results are also of interest as reactions to musical stimuli; since the performer is obviously subjected to the stimulus of his own music. Two items of interest appear, viz., that in executing a series of notes, the final one is struck with most energy, and that in rapid playing there is always a diminution in the intensity of the movement. Speed and force vary inversely. Binet and Courtier (6) also investigated the influence of musical stimuli on respiration and circulation. The former was studied by means of the pneumograph; the latter by the plethysmograph. The reactions of a single subject were studied. In one series of experiments, isolated tones, chords, and musical exercises, possessing no intellectual or emotional associations were used. These sensorial stimuli, as the authors term them, produced no respiratory modifications, except an acceleration of from .5 to 3.5 additional respirations per minute. The acceleration varies directly with the liveliness of the movements, and is greater for the minor mode and discordant sounds. Musical selections, chiefly songs, arousing emotional associations, according to the introspections of the subjects, vary in their influence according to the affective tone evoked. Sad melodies accelerate respiration by 2.6 on the average, considerably diminish the amplitude and produce irregularities in both acceleration and amplitude of respiration. Gay music, *e.g.*, military marches, produced an acceleration of 3.8 and showed less tendency to reduce amplitude. A third class of melodies evoking complex and unclassifiable emotions and including fragments of "la Rencontre," "laisse moi contempler ton visage"; "Walkürenritt," etc., produced an acceleration of 3.3 per minute with a tendency to reduced amplitude. With respect to circulation, purely sensorial excitations produce a slight lessening of the amplitude of pulsation. Dissonances produce a greater effect of the same nature. Sad music has almost no influence, while gay music nearly always provokes a reduction.

The rate of the pulse remains constant or increases, in the case of simple musical elements, as much as 6. Melodies produce an acceleration ranging from 0 to 15. Finally, gay music produces an accentuation of dicrotism, sad music the reverse. A selection from the two series of tabular reports is embodied in the following combined table:

Music	Action or Respiration R. Before	After	Action on Pulse R. Before	After
Marche Triomphale, Tannhäuser......	9.6	13.5	84.	84.
Marche de Faust, Gounod............	9.	12.5	81.	87.
Marche Hongroise, Berlioz............	11.	14.5	86.5	91.5
L'Epée (chant), Wagner..............	9.5	14.	69.	70.
La Chauvauchée (chant), Wagner....	9.	14.	68.	83.
Printemps (chant), Wagner...........	9.5	13.	69.	73.5
La Rencontre (chant), Faust..........	10.5	13.	68.	84.
Laisse moi contempler (chant)........	11.	12.	73.	83.

Thanks to an interesting clinical accident Dr. Patrizi (**29**) of the University of Modena was able to carry out important experiments on the influence of music on the cerebral circulation. Emanuel Favre, a boy of thirteen years of age, was severely wounded in the head by an axe. The wound was 13 cm. in length, cleaving the bones of the skull for the entire distance. Restored to health the boy presented a soft cicatrice, through which the pulsations of the brain were plainly visible, and changes of the cerebral circulation could be accurately determined. Two questions presented themselves: Is the circulation of the blood in general influenced by music? Is the circulation of the brain more or less influenced than that of the rest of the body? Plethysmographs were used to determine changes in circulation; for registering the pulse of the brain a cup of gutta percha was made with an electric connection capable of showing the slightest modification in volume or pulsation. The results were recorded on a kymograph. In general it was found that pulsations took a higher range after a musical note, or a very near repetition of the same note. High notes produced greater changes than tones of low pitch. For isolated tones three conditions were observed: The volume of the pulse in the arm increased in the same proportion as that of the brain; the two varied inversely; the volume of peripheral circulation remained constant while that of the brain increased. The same reactions accompanied melodic stimuli. Five tracings taken at considerable intervals showed that an increase of cerebral circulation occurs under the influence of simple music. This effect is more or less marked and persistent, according to the nature of the melody (pathetic or martial) and the movement (slow or fast). Every increase in pulse was related to musical stimuli, and the different amplitudes of the variations observed, seemed to depend on the relative intensity of the musical stimuli. Both cerebral and peripheral

circulation were increased by the singing of the Marseillaise, while a polka augmented the cerebral circulation and diminished that of the arm in the same proportion. A "galop" increased the cerebral circulation, while that of the arm remained unchanged, and great mental lucidity accompanied the music.

The depressing or exhilarating character of the music did not correspond to the deviations of the plethysmographic curve, and the circulatory effects were found to be independent of the respiratory. Patrizi is undecided whether the variations in cerebral volume are autonomic neuromuscular functions, or passive reflections of general vasomotor phenomena. He points out, however, that his results diverge from those of Feré and Tarchanoff, since the substitution of lively for melancholy music did not produce an increase of volume as these investigators assumed. In a book published several years later, Patrizi (**30**) enlarges on the observations reported above in the effort to establish a musical esthetics founded on physiological principles.

G. E. Ferrari (**17**) repeated the experiments of Patrizi on normal, feebleminded, and idiotic individuals, devoting himself to the special study of the modifications of capillary circulation under the influence of music. He concludes that there are vasomotor activities after a musical emotion only when the individual is in a state of psychological inferiority; when the superior psychic functions have vanished; and when mental coördination ceases to inhibit emotion. In short, the effect appears when an organic disorder exists.

Dr. Guibaud (**20**) followed up Binet and Courtier's study of the effect on respiration and circulation by very methodical investigations along the same lines at the University of Bordeaux. His method and apparatus were similar to Binet's, but were not confined to the study of a single individual. His conclusions quoted "in extenso" in a review by Binet, include the following: All individuals do not react in the same way to similar musical stimuli, whether simple or complex. Moreover, some subjects react to every kind of musical stimulus, while others react only to certain ones. In the first series of experiments, simple tones, chords, and gammes (scales) were used. It was found that reactions to dissonances were more marked and frequent in the majority of cases, although subjects habituated to modern music and its bold use of dissonances did not so react. For isolated tones, the extent and frequency of reactions are functions of pitch, grave sounds being more effective than acute. Minor chords produce more effective and intense reactions than do

majors. Reactions to gammes are in general less accentuated but minors provoked the greater number of responses. There is no uniform type of respiratory modifications for any of the types of musical stimuli named, but the respiratory rhythm is now accelerated, now retarded, according to no apparent law. In the same individual, however, the variations are constant in direction for the same stimuli. The amplitude of respiration may or may not be modified, but when it is it usually becomes deeper. The total pulse determined by plethysmographic, not sphygmographic, tracings does not present perfectly definite changes. Where reaction appears it is vasoconstrictor. The amplitude of the pulse is diminished, the summit rounded, and details become less apparent. The cardiac rhythm is in general accelerated, but often by an amount only appreciable by comparing a dozen pulsations before and during the stimulus. Vasoconstriction varies in intensity with the nature of the stimulus and subject. The fall of the plethysmographic curve occurs during emotion and varies directly with its intensity. A comparison of respiratory and circulatory modifications shows that the former always precede the latter by 3 to 4 seconds. When musical selections are used as stimuli, vascular and respiratory reactions become still more varied. Inconstant in direction, they follow the evolution of the melody in a given individual. In the course of a musical selection, the respiratory reaction passes through diverse phases. Respiration is regular when the melody is calm, and becomes irregular, sometimes deep, sometimes shallow, when rhythm or intensity is modified. The rhythm of respiration tends to adapt itself to the rhythm of the music especially when the latter grows slower. The intensity of peripheral vasoconstriction depends upon individual impressionability. With a constant uniform rhythm, the plethysmographic curve rises gradually and the pulse which was reduced (in amplitude) during vasoconstriction resumes normal amplitude. Variations in rhythm occasion fresh phenomena of vasoconstriction; *i.e.*, a fall of curve with diminished amplitude of pulse. Finally the vasomotor phenomena agree with the feelings experienced by the subjects during musical stimulation. Mention should likewise be made of the work of Darlington and Talbot (**46**) who experimented on the influence of music on the discrimination of lifted weights. There were three subjects, and the stimuli consisted of 25 musical phrases of 9 notes each, played upon the piano. The music was in 2–4 time, in the key of C, and ranged through 5 octaves. The authors conclude that music facilitates attention, the middle octaves having the greatest influence. In opposi-

tion to Feré's results, they find that there is no essential relation between pitch and dynamogenic effect.

In 1898 also appeared the striking investigation of De Rochas (**32**) on the influence of music on behavior during hypnosis. An earlier series of similar investigations is, howeved, reported by Dr. A. S. Warthin (**44**) (1894). Dr. Warthin, observing what he considered as an autohypnotic condition in certain enthusiastic attendants at a concert of Wagner's music, attempted to determine experimentally the influence of music on a group of hypnotized subjects. There were seven subjects, five men and two women, representing a wide range of individual character within limits apparently normal (p. 89). Pulse tracings were taken by means of appropriate apparatus, and the suggestion was given to pay attention to nothing but the music and to retain the memory of the effect on awakening. The first subject was a physician of emotional nature and easily hypnotizable. When Wagner's "Ride of the Valkyries" was played on the piano, the pulse of the subject was accelerated, while the amplitude and the pressure increased. As the music continued the pulse rose from a normal of 60 to 120 per minute, becoming quick full and of low tension. The rate of respiration increased from 18 to 30 per minute, body and limbs showed excitement (agitation), the entire body being bathed in perspiration. On awakening the subject reported that he did not perceive the music as sound but as a sense of falling.

Other selections gave similarly striking results. The fire music in the closing scene of the Valkyries increased pulse rate with greater fullness and less tension. Slow music (Walhalla motive) produced first a retardation of the pulse and an increase of tension. Later the pulse rate was almost doubled while tension decreased. The music of the scene in which Brunhilde summons Siegfried to Walhalla produced irregularity of respiration and decreased the rate and amplitude of the pulse. Respiration became slower and cold perspiration appeared. Marked effects were also produced by isolated musical elements, certain chords (Tannhäuser) always producing violent and painful muscular contractions, while others provoked hyperesthesia that resisted suggestion of anesthesia. The chord of B minor loudly played reduced pulse from 120 to 40 and evoked cold perspiration and chills. The same experiments were repeated with the other subjects with similar results, although the others were less emotional and in some cases ignorant of the identity of the selection. Music was also found to facilitate the hypnotic process as Braid and Moll assert in their manuals.

The experiments of Alfred de Rochas (**32**) were carried out on a well known French model, while the latter was in the first phase of the hypnotic condition. The author experimented very methodically, making successive use of isolated tones, intervals, chords, scales, phrases and entire musical selections by different composers of contrasting types. According to De Rochas, isolated tones provoke a trembling extending to all parts of the body, the character of the reaction varying with the pitch and intensity of the notes. Very high tones evoked the expression of pain, very low tones that of anguish or terror. Chords excite the same reaction as simple tones, and dissonances at any part of the scale produce an expression of suffering. The ascending scale brings out a series of reactions mounting from foot to head, while descending scales produce reactions of the same type but in reverse order. These phenomena occur in all tonalities. March and dance music involving both melody and rhythm produce very complex movements, the gestures of the upper part of the body corresponding to the melody, those of the lower part of the body corresponding to the rhythm. The music of different composers provokes varying effects. That of Gounod is most effective in producing appropriate expressive mimicry, physiognomical changes, gestures and attitudes. The music of Wagner and Beethoven is less effective in this respect. Verdi, especially "The Miserere," exercised a remarkable influence, while the "Derniere pensée" of Weber and the "Berceuse" of Reber evoked appropriate attitudes. The author concludes that the phenomena observed are pure reflexes, set off by auditory stimuli, without the intervention of the will.

De Rochas (**33**) later presented an amplified version of his experiments and conclusions in a book reviewed favorably by L. Dauriac (**11**, 1900). The latter regards De Rochas' work as a distinct forward step in the study of musical reactions, but advises caution in the use of the hypnotic method, and in the acceptance of data derived from this source.

In 1899 Dr. Herbert Dixon (**12**) reported the repetition of experiments similar to those of Dogiel with similar results, *i.e.*, increased blood pressure and cardiac action, and variations in respiration. He does not state his method or the number and nature of his subjects, but mentions several physiological effects of music which are not reported by other experimenters. Among such effects are "perspiration, desire to micturate, lachrymation and, rarely, laughter."

Dr. J. T. R. Davison's (**10**) article on music and medicine,

appearing about the same time, summarizes and confirms Dogiel's and Tarchanoff's experiments on the physiological effects of music. On the basis of such experiments the theory is advanced " that music exercises its influence over the body, without the influence of the highest nervous centers," and that the human organism participates in the tendency to vibrate synchronously with music which sometimes obtains in the animal world," a theory which in a modified and improved form has been restated recently by Dr. Beaunis (**2**).

During the same year, Urbanschitsch (**42**) described some rather indefinite experiments on the influence of tonal stimuli on handwriting. The subjects of the experiment were requested to write the same phrase three times in succession. The first test was conducted in silence. During the second, a low tone was produced; during the third, an acute tone by means of a harmonica. The tones of low pitch produced in many subjects an enfeeblement of tonus in the scriptorial muscles, resulting in a tendency to uncertainty and increased size of handwriting. Tones of high pitch, on the contrary, increased muscular tonus, the writing was more stiff and smaller, and the letters were more angular. Dots of the i and accents were often omitted. One of the subjects showed a tendency to write above the line during high tones, while low tones caused his writing to descend. The number of subjects is not stated and the examples offered of changed handwriting are indefinite.

In 1902 Vaschide and Lahy (**43**) published a critical synthesis of the work of preceding investigations as a preparation for subsequent work. In this work they analyze the value and significance of two important coefficients of organic response, viz., circulation and respiration. The results of preceding investigations are condensed into two tables, the first showing the influence of musical elements on organic reactions, the second exhibiting the variations of organic functions in relation to the general effects of music. The general conclusions are: (1) The repetition of a musical stimulus diminishes and finally banishes peripheral reactions; (2) musical rhythm produces by suggestion a mechanical action in respiration. In general respiratory rhythm follows that of the music, increasing or diminishing with the latter, without going beyond the limits of extreme variations; (3) melody only produces variations in organic functions, in so far as one of its elements—rhythm, mode, timbre, intensity—changes (Guibaud); (4) monotonous melody produces no peripheral changes. Scales provoke weak reactions, because they are the most simple and

monotonous melodies; (5) the brain temporarily increases in volume in response to all musical stimuli.

The earlier experimental work of Professor Feré has been treated in preceding sections. Fere's total contributions to the experimental study of response to music exceed in volume those of any other investigators. Appearing originally as communications to the *Societé de Biologie* of Paris, they were assembled in two chapters of a very interesting book published by Feré (16) in 1904. Feré confined his material and methods within severe limits. The auditory stimuli were never musical selections but always musical elements and the apparatus employed in all the later experiments was the Mosso ergograph. In short, Feré's experiments constitute an exhaustive study of the influence of elementary musical stimuli on muscular work and fatigue. The musical stimuli employed include simple, successive, and alternating intervals, dissonances, ascending and descending series of tones, and alternating tonalities and rhythms. The experiments were from a purely behavioristic standpoint, Feré stating that the consciousness of the subject is not relied on and that the fact of musical deafness in some subjects raises the value of the results. The subjects raised a weight of three kilograms with the middle finger in time with the metronome, and the erographs were separated by pauses of a second and grouped in series, which series were in turn separated from each other by five minute intervals. From four to thirteen experiments were made on each element. As a result of these experiments, certain intervals were classified as depressive: viz., la, sol(flat), la-si, la-do, la-mi(flat), la-sol; and others classified as exciting: viz., major third, fourth, fifth, minor sixth, major sixth, and the octave. After fatigue appearing gradually during the stimulation by exciting intervals, the sudden change of interval to a depressing one resulted in a great accession of muscular energy. The alternation of intervals, *e.g.*, octave and major seventh, major and minor seconds, etc., produced oscillations of energy, with progressively increasing muscular energy upon each recurrence of the exciting interval. The alternation of a sol(flat) and si seemed to indicate that the first tone increases the exciting effect of the second, while its own effect is decreasing. Two chords in alternation produce a very different effect if the fifth, the tonic or the dominant third of the initial chord form part of the second. The exciting value of alternating chords decreases with the distance between them. All minors are not depressing (do sharp) nor are all major chords exciting (re flat).

Feré not only experimented upon fresh subjects, but also on the fatigued. The musical elements are then found to change their rôle. Depressing tonalities or alternations, etc., become exciting and *vice versa.* Finally Feré found that the series of tones acts differently on work, according to the direction from grave to acute (ascending) or from acute to grave (descending). The total work done in a set of experiments under the stimulus of ascending sounds is greater than for a descending series of sounds, in the ratio of nearly two to one in the cases examined.

The work of Feré cannot be summarized conveniently, as he himself declares, and the above is nothing more than a selection of the salient points, in a work that deserves much attention and suggests many problems. Since the appearance of Feré's work, the few experimental studies of reactions to music that have appeared are due to American investigators. Miss Gamble (**19**) (1905) in a study of attention and thoracic breathing devoted a group of experiments to the effect of music on respiratory activities. There were twenty subjects and organ music of three kinds was employed as stimuli, viz., (1) hymns such as "Vox dilecti," (2) two chorals from Mendelssohn, (3) elaborate compositions ranging from "The Torch Light Dance," of "Brides of Cashmere" of Rubinstein, to the "Dead March" in "Saul." The apparatus used was the Sumner belt pneumograph, and the revolution time of the drum during the musical experiments was 3.33 minutes. The author says the results are of doubtful validity. Out of 155 stimulus tracings in which modifications of regularity were studied, the expiratory pause became less regular than the corresponding normal pause in 25.2 per cent and more regular in 14.2 per cent and the amplitude became less regular than the corresponding normal amplitude in 24.5 per cent and more regular in 12 per cent. The tendency was toward irregularity and the length of the pause seemed affected by variations in loudness. It is suggested that respiratory functions are influenced by musical rhythms.

In a subsequent study devoted solely to the influence of music on respiration, Miss Gamble (**18**) seeks to determine the differences in the effect of (1) music in the major and minor keys, and (2) loud and soft music. The subjects were 29 college women, and the apparatus used was the same as in the preceding experiments. Organ music was employed as stimulus. Four series of experiments were carried out and the results may be summarized as in the accompanying table:

Series	Number of Subjects	Music	Normal Breathing Rate	Average Rate for Major Passages	Average Rate for Minor Passages
I	8	4 hymns	16.7	18.9	17.8
II	12	2 Mendelssohn	16.9	18.2	16.8
III	6	9 elaborate compositions used in fixed order....	18.3	18.3	17.2
IV	10	18 elaborate compositions	18.7	21.3	21.5
		Loud 21.8	Loud 21.9		
		Soft 20.8	Soft 21.3		

A table showing the percentage of changes in the amount and regularity of amplitude and respiratory pause, under the influence of loud and soft major and minor music fails to disclose any significant alteration.

The author concludes: (1) that listening to music, loud or soft, major or minor, tends to shorten the expiratory pause, and to make the breathing faster and shallower; (2) "no remarkable difference appears in the effect either of loud or soft or major or minor music"; certain tendencies appear, however, viz., (a) loud music tends to decrease, and soft music tends to increase regularity; (b) loud music is more effective in shortening the respiratory pause accelerating breathing and making breathing shallower; (c) music in the major key had the greatest effect on the respiratory pause, and the major passages showed a greater tendency to produce acceleration; (3) musical stimuli do not markedly affect the regularity of respiration.

J. P. Shepard (36, 1906) in an exhaustive study of organic changes and feeling carried on during three years used musical stimuli (chords and selections) in certain of his experiments. The recording instruments employed were the pneumograph, and plethysmograph. In the preliminary series of experiments, there were six subjects, including the author, while another series of experiments were conducted upon an individual with a pulsating cerebral scar, similar to that of Patrizi's subject. Hundreds of tests were made and the following are cited from the preliminary tests:

Experiment	Stimulus	Results	Introspection
B 27	Major Triad (CEG)	Fall of volume and lower pulse	Pleasant
W 77	Major Triad	Fall of volume and lower pulse	Agreeable
B 6S	Sousa's Band	Fall of volume and lower pulse	Agreeable and stimulating
W 76	Major Triad	Fall of volume and lower pulse	Exciting
W 96	Major Triad	Fall of volume and lower pulse	Exciting

In the subject with the pulsating scar over the brain, a musical chord produced a decrease of volume of the hand and pulse, while the volume and pulse of brain increased. In the final series of experiments on the rate of the heart and breathing, there were five subjects and seven trials of music which, according to the subjects' introspections, were agreeable and exciting. All resulted in a faster rate, with generally a marked increase. In four cases of agreeably depressing music, the pulse time was shortened. In 16 out of 18 cases in which the stimulus was a loud, unexpected whistle of a disagreeably exciting nature, there was an acceleration of the pulse. Agreeably exciting or depressing music also tended to eliminate the Traube-Hering blood pressure wave and the rate of breathing was almost always increased for all stimuli.

Miss M. Johnson (**25**) in a study of the relation of music to physical education states that the strength of muscular contraction increases with the intensity and also with the pitch of sounds. It is greatest when sound and movement are simultaneous. Music postpones the point of fatigue, but is not favorable to steadiness of movement. We are not told, however, whether these conclusions are based on experiments, observations, or are theoretical generalizations, founded on the experimental work of Feré, Tarchanoff, Yerkes, etc.

Dr. Ingennieros (**24**, 1907) presents an excellent critical review of the psychophysiology of music up to 1905. The bibliographical references to the experimental work of Italian scientists in this field are particularly valuable. The treatises of Patrizi and De Rochas based on the experimental studies referred to earlier in this review are analyzed and criticized in detail, and several rarely observed effects of musical stimuli are recorded. Such are the influences of music on the movements of a foetus, quoted from Vaschide, and Albertotti's report of a case of congenital blindness in which the subject saw phosphenes on hearing the sound of a siren, the result of involuntary contraction due to hypertonus of the ocular muscles.

L. Azoulay's recent case of an artist who uniformly experiences a flash of white light on hearing certain tones may be similar to that of Albertotti's congenitally blind subject, cited above. Azoulay, however, is inclined to classify it as a primitive form of synesthesia (**28**).

W. V. Bingham (**7**, 1910) devotes a chapter in his *Studies on Melody* to the effect of melodic stimuli upon muscular movements. The voluntary movement studied was the tapping movement of the index finger of the right hand. This movement was recorded in all

its details by connecting the finger (by a silk thread running over a pulley) with a rubber thread bearing an aluminum writing point held against an electrically driven revolving drum by the torsion of the thread. Simple melodic stimuli—tones, intervals, and groups—were afforded the subject by a reed organ in electrical connection with a marker which recorded the instant of depressing and raising the keys. The duration of each stimulus was three seconds. There were nine subjects and introspective accounts of affective tone were compared with objective achievement. With simple auditory stimuli (tones and noises) alterations of the natural tapping rate, usually retardation, occurred. In the case of melodic stimuli, the results are different and difficult to summarize. The tables, however, show characteristic accelerations and retardations varying with the direction of the interval (ascending or descending), the extent, the relation of consonance, and dissonance, and the mental states of the subject, including expectation at the beginning and a sense of completeness or incompleteness at the end of a sequence.

L. P. Ayres (**1**, 1911) reports the influence of music on competing athletes as shown by tests carried out during a six-day bicycle race. The tests covered 46 miles, one-half of the miles being ridden while the band played, the other half in silence. In two series of tests the average time per mile was decreased, while the rate increased, during the period of musical accompaniment.

H. P. Weld (**45**, 1912) presents interesting plethysmographic and pneumographic data on reactions to music in connection with a study of musical enjoyment. No tables are given since the object of the experiments was that of controlling introspections. The general results of the work are, however, indicated. Eight subjects took part in the experiments, which were carried out by means of the plethysmograph and two Sumner pneumographs for recording thoracic and abdominal respiration, the stimuli being a variety of selections on the phonograph. The author notes that the act of listening to music was attended by a decrease of the volume of the forearm in 90 per cent of the cases, the decrease occurring almost immediately within two to five heartbeats after the music began. In auditors of an active temperament the initial drop in the volumetric curve is abrupt, in those of a phlegmatic type it was gradual. "The heart rate was usually accelerated, the acceleration beginning during the first few seconds and persisting throughout the music." There was in general no correlation between the tempo of the music and the change of heart rate, even the slowest musical tempos, which were

much slower than the normal pulse, produced an acceleration of pulse and the most rapid tempos had no more accelerating effect *per se* than the slowest tempos." The author therefore ascribes this effect to the influence of expectation during the fore period. There is no correlation between heart beat and musical rhythm. The most striking changes in the respiration of the music period are acceleration of rate, and irregularity of amplitude, while in certain auditors respiration became more shallow. There is no correlation between musical tempo or phrasing and respiration. In certain auditors changes in respiration were the most striking physiological effects. The respiration of auditors characterized by active attention became more rapid and shallow during the music period, while in the emotional auditors, breathing was very irregular in rate and amplitude, respiration tending to follow the music. The results agree with those of previous investigators, except those of Mentz, who made a statement that the pulse is retarded in passive attention, which Weld is unable to admit.

G. Stepanov (**37**, 1915) studied experimentally the influence of an auditory stimulus on dreams. The experimenter and his subject occupied adjoining rooms and at intervals during the night the former played musical selections (chiefly from Wagner) on the piano, awakening the subject who reported her dreams through a speaking tube. Interesting facts were brought out, the behavior of the subject demonstrating that there was subconscious recognition, although when awakened she could not name the selection. In some cases, in her knocking on the wall of the room to announce her presence at the speaking tube, the blows corresponded in time to the tempo of the music. In many cases dreams of a non-musical or even non-auditory character were evoked, indicating transformation of acoustic stimuli into visual images, a phenomenon analogous to synesthesia. It would be interesting to experiment in this way on subjects of synesthesia to see if sensory stimuli evoked dreams in terms of the fixed sensory associations.

In 1916 Ribot (**31**) called attention to the fact that deaf-mutes, notably Helen Keller and Laura Bridgman, respond to musical sounds. They distinguish several different musical instruments according to the nature of the vibrations, appreciating in this manner, pitch, rhythm, and measure. Motor reactions to tones appear to occur independently of auditory stimulation.

For similar reasons and also on theoretical grounds, Dr. Beaunis (**2**) in a recent essay on musical emotion concludes that we may justly

regard the organism as a system susceptible to vibrations, and that when sonorous waves strike it, we must admit an effect on general sensitivity as well as auditory stimulation in the proper sense of the term. Certain parts of the organism such as bones and teeth are admittedly capable of vibration, but Dr. Beaunis thinks that the other tissues also when living are in a state of tension which renders them capable of sympathetic vibration to sound stimuli. The various organs and systems of tissue are like so many resonators, each responding most effectively to some particular tone; hence the apparent localization of deep tones in the abdomen, and the high tones in the head, which may be established by introspection. The very adjectives "high" and "low," as applied to tones of great and small frequency, point to a common experience and is reflected in the vocabularies of seemingly unrelated languages. This localization is based apparently on mere analogy, but may have a physiological basis in the varying vibratory rates of the tissues. Thus vibrations of the tissues swell the volume of organic sensations and thus contribute an important element to musical emotion.

One of the most recent studies of physiological responses to music is that of Hyde and Scalapino (**23**, 1918) who investigated the influences of music upon electro-cardiagrams and blood pressures. The reactions of one subject checked by the reports from two others are given in the preliminary experiments, so far published. The cardiagrams were recorded with the Einthoven string galvanometer, while pulse and blood pressures were determined by a modified form of Erlangen's sphygmomanometer. The musical selections employed were Tschaikowsky's "Death Symphony," the "Toreador Song" from "Carmen" and Sousa's "National Emblem March." The symphony produced a slight increase of blood pressure, pulse rate, and amplitude. The Toreador song accelerated pulse rate, and lessened amplitude. The after effects were increased, systolic pressure, pulse pressure, and pulse rate, with a decrease in the action current of the ventricular contraction. The March resulted in slower rate, and increase of systolic pressure, pulse pressure and action current. Minor tones increase pulse rate and action current and lower systolic and diastolic pressure, while stirring music increases systolic and pulse pressures. The data are arranged in tabular form exhibiting the changes indicated. Further experiments have not yet appeared.

During the past two years the writer of this review has been conducting experiments on the influence of music on behavior. These

experiments were mainly planned to test voluntary activities, a few experiments on affective processes being introduced to test the specific effects of the particular selections employed. The music was produced by means of an Edison cabinet phonograph. The selections employed were chiefly instrumental and classical. From 10 to 20 subjects of both sexes served in each experiment, and the activities tested include work with the dynamometer, aiming, typewriting, handwriting, suggestibility (according to the method of Sidis), choice of colors, response to the Müller-Lyer illusion, involuntary movement, respiration, and the psychogalvanic reflex. The general method consisted in a comparison of the various activities with and without musical accompaniments, due precautions being taken to avoid suggestion or practice effects. The general conclusions from our experimental work are as follows: 1. music tends to reduce or delay fatigue and consequently increases muscular endurance; 2. music has no definite effect on precision or accuracy of movement, if the rhythm is not adapted to the rhythm of the work; it reduces accuracy in typewriting and handwriting, the result being shown in an increased number of errors; 3. music speeds up such voluntary activities as typewriting and handwriting. It also accelerates respiration; 4. music increases the extent of muscular reflexes employed in writing, drawing, etc.; 5. music reduces normal suggestibility, except in the case of direct suggestion involving color in which suggestibility is increased; 6. music seems to have a tendency to produce a shift in normal preference for chromatic and achromatic impressions, the change being toward the blue end of the spectrum and the white end of the achromatic series; 7. music has a tendency to reduce the extent of illusions by acting as a distracting factor; 8. music influences the electrical conductivity of the human body as manifested by increased flucuations in the psychogalvanic reflex.

Our review of the experimental literature of behavior in response to musical stimuli has been fairly complete, and all that remains is to determine the amount of agreement appearing in the results of the various experimental studies. The fact is, of course, evident that music profoundly influences physiological reactions. But the direction and reciprocal correlation of these reactions are still matters of dispute in many cases.

Writing in 1907 Dr. Ingegnieros (**24**) declared that three conclusions follow from the experimental work up to that time; viz., 1. musical stimuli like all sensorial excitations determine an increase of the general physiological activities of the organism; the influence

of music is a fact experimentally demonstrated; 2. musical stimuli in certain well known conditions determine in the organism the transient functional reactions which characterize an emotion; 3. physiologically there do not exist functional reactions which are specific of the musical emotion; it is a matter of reactions common to emotions in general determined by music in certain conditions.

To-day we may perhaps summarize the results of researches in somewhat less general terms.

The following points now seem generally agreed upon: music

1. Increases bodily metabolism (Tarchanoff, Dutto).
2. Increases or decreases muscular energy (Feré, Tarchanoff, Scripture).
3. Accelerates respiration and decreases its regularity (Binet, Guibaud, Weld).
4. Produces marked but variable effect on volume, pulse, and blood pressure.
5. Lowers the threshold for sensory stimuli of different modes.
6. It thus affords the physiological basis for the genesis of emotions according to the James-Lange theory and consequently influences the internal secretions according to the researches of Cannon.
7. The precise influence of different modes and types of music has not been determined and waits upon an adequate classification of musical selections, which must probably proceed at first by introspective and statistical methods.

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THE NATURE OF INSTINCT

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The present review is intended more as a critique than as a summary. The decade 1913-1922 inclusive has witnessed an active promulgation of instinct doctrines, and it is the delineation of what seem to the reviewer the significant directions of this doctrinal movement that will constitute the task of this abstract.

The literature on instinct falls into three classes: (1) observational studies (field or laboratory) upon humans and animals (mostly the latter); (2) applications of instinct theory to social phenomena, and (3) direct studies in instinct theory itself. It is the last type of contribution that will mostly concern us. But we shall have recourse to the observational literature and to the literature of social speculation as well, wherever these latter seem to offer important grist for the theoretical mill.

We may begin with a brief summary. Two main types of theory appear throughout the decade. The first we will call the *teleological* type of theory. This defines and identifies instincts in terms of *ends.* These ends themselves, however, it may define in different ways: biologically, mentalistically, or behavioristically. The term teleological is used here in a purely *descriptive,* non-metaphysical, sense. A teleological theory as we use the term may be ultimately mechanistic, provided merely that its immediate descriptions are in terms of *ends.* These ends, themselves, may be finally reduceable to purely naturalistic phenomena. A biological type of teleological theory is to be found wherever recourse is had to biological ends in constructing the list of "instincts" or of "fundamental behaviors" (Holmes, **28**). Few teleological theories are completely void of some such biological cast. Examples of the primarily mentalistic types are those of Drever (**21**) and McDougall (**38**); while ones of the primarily behavioristic type are to be found in Woodworth (**59, 60**), Craig (**19**), Dunlap (**24**), Tolman (**52**).

The other main variety of theory which has run throughout the decade may be called the *reflex* variety. This identifies and defines instincts primarily in terms of the direct sensori-motor mechanisms

(reflexes) which they involve, instead of in terms of the ends which they subserve. It has two sub-types: A, the type which conceives the instincts in terms of relatively *simple* sensori-motor connections; and B, the type which conceives them in terms of complex sensori-motor *patterns*. The first may be called the *simple* reflex theory, the second, the reflex *pattern* theory. The purest example of A is to be found in Loeb (**37**), and of B in Watson (**54**). (Thorndike's theory (**50**), though not so clear an example, also belongs to type A.)

Various admixtures of all types of theory have, of course, been prevalent. For example McDougall's classical doctrine in the *Social Psychology* (**38**) was really such an admixture, as he himself now admits. It combined a mentalistic variety of the teleological theory (instincts defined in terms of emotions) with a reflex theory (instincts defined in terms of their characteristic initiating stimuli and characteristic resultant responses).

Toward the latter part of the decade, partly as a result of the new animal studies which emphasized the extreme flexibility of animal behavior (the Peckhams,**44**; Whitman,**57**) and partly as a result of the excesses in the matter of instinct-manufacture committed by the sociologists (v. the criticisms of Faris,**25**, and Bernard,**20**), there has developed a lively critical movement. This has been directed at almost every one of the above types of theory. It is primarily this critical movement and the resultant reformulations of theory which it has evoked that form our main point of attack.

The nature and scope of the critical movement can, perhaps, be summarized under the five heads: (1) nonvariability, (2) arbitrary grouping, (3) "preëstablished harmony" and "innate ideas," (4) faculty psychology and (5) fundamental instinct-habit identity. Of these (1) and (2) arose directly from the animal studies and from the excesses of the sociologists, respectively. The remaining three are of more purely theoretical and methodological origin.

(1) *Nonvariability.* This is the charge which develops directly from animal studies. It calls attention to the extreme *flexibility* of most actual animal behavior. It asserts that nothing like real *reflex patterns* are to be found anywhere in nature. The solitary wasp, ammophilia, does not sting her caterpillars always in exactly the same segments with exactly the same degree of resultant paralyzation (the Peckhams,**44**). Birds do not build their nests by means of a precise and *invariable* order of movements (Swindle,**49**). Indeed these and countless like observations have given the pure reflex

pattern theory its final *coup de grace*. As Professor Hocking (**32**, p. 39) says: "It may be admitted at once that the explanation of instinctive behavior by the chain-reflex pattern has definitely broken down . . . The most obviously instinctive behavior, such as nest-building in birds, is too irregular in its progress, permits too many interludes and divertisements, alarums and excursions. A chain-reflex should have an invariable order-process. A should always come before process B, because its conclusion is necessary to set process B in motion. If by accident, process B is set off first, it will never go back to A, but will proceed mechanically to C and D. If, per contra, there appears to be a degree of liberty, so that ABC may be performed as well in order BAC, or even CBA, the chain-reflex needs some outside assistance." Kuo (**34**), Bernard (**20**), Ayres (**17**) and Kantor (**33**) have all indicated this breakdown of the complex reflex-*pattern* theory, at least for humans.

(2) *Arbitrary grouping*. This is the indictment which has arisen most obviously because of the excesses of the sociologists and economists. (See, for example, Parker, **43**.) The charge asserts that "the" instincts are mere arbitrary groupings of responses, determined by the special interests or purposes of their authors. Note, say the proponents of this charge, the innumerable different lists which appear in the texts, and note the new and remarkable instincts which the sociologists and the educationalists are constantly offering us, *e.g.*, the "instinct of workmanship," the "religious instinct," the "instinct of make-believe," etc. Practically all of the "attackers" have dwelt upon this charge, Allport (**15**), Ayres (**17**), Bernard (**20**), Dunlap (**22**, **23**), Kantor (**33**), Kuo (**34**). The charge is levelled most directly at the teleological type of theory. And there is no doubt justice in it in the sense that our "lists" in the past have been largely constructed in such a way. But the remedy is to be found merely in more careful definition and better empirical observation. All we need is, as Dunlap himself suggests, a more "truly psychological grouping" (**22**, p. 309).

We turn now to the more theoretical methodological objections:

(3) *"Preëstablished harmony"* and *"innate ideas."* This charge consists in declaring that instinct-doctrines imply a sort of Leibnitzian preëstablished harmony between the organism and its environment, both in the matter of having the appropriate perceptions ("innate ideas") and in that of making the appropriate responses in the presence of these perceptions. The charge has been made most specifically by Bernard (**20**) and Kuo (**34**). We may

quote the latter: "To assume any inborn tendency is to assume *a priori* relation between the organism and stimulating objects . . . Such an assumption is no less objectionable than that of innate ideas. As a matter of fact, both the theory of instinct and that of innate ideas are based on the same conception; namely the conception of *a priori* relation of the organism to external objects. If it is true that one cannot have an idea of a tree before one has actually seen or learned about a tree, it must be equally true that one cannot have any food trend before one has ever eaten food."

The charge seems more brilliant than cogent. For, accepting evolution, the notion of at least *some* amount of preëstablished harmony (including innate perception) becomes not only permissable but inevitable. Indeed, preëstablished harmony is the very essence of that which evolution is supposed to accomplish. We accept the notion of preëstablished harmony in the matter of sense organs and response organs; why not also in that of the behavior that results from such organs? It is no more difficult to conceive that amount of preëstablished harmony which results in (albeit, on the first occasion, in a haphazard way) the building of a nest, than to conceive that amount to be found in the wings, the eyes, the bill, the feet, etc., of the organism building the nest. As McDougall says: "It remains a question of empirical fact." (**39**, p. 297.) And Geiger (**27**) in particular has been at some pains to show the fallacy involved in the charge.

(4) "*Faculty psychology.*" This is an indictment which has been made particularly by Field (**26**) and Allport (**15**, **16**) but also implied by Ayres (**17**), Bernard (**20**), Dunlap (**22**, **23**), and Kantor (**33**).

The assumption of instincts it is said, is similar to the now discredited assumption of mental faculties. For the "instincts" are mere *class names* which the instinct-psychologists have elevated to the rank of potencies. The charge is levelled most directly at the teleological theories. These theories are said to assume mystical drives or forces behind the actual responses. But the assumption of such forces adds nothing to a causal and descriptive explanation of the phenomena.

Is the indictment sound? Are the teleological definitions, in any vicious sense, a return to faculty psychology? It would hardly seem so. For how else can we so *simply* and *easily describe* such empirical facts as that with constant environmental conditions, one and the same external stimulus will, in the same individual, on one occasion

arouse one response and on another occasion another quite different response, and that one and the same stimulus will arouse two quite different responses in two different individuals, except by assuming varying degrees of instinctive proclivity. For as McDougall points out: "the correct ascription of an action, or a phase of behavior, to a particular instinct *enables us to forecast the further course of behavior*" (38, p. 313). (Italics mine.)

It is because there *are* observable functional interdependencies, common waxings and wanings, common references to preceding causal excitement, that we find the concept of instincts a simple and useful descriptive tool. In one individual, we say, a given instinct is relatively weak, in another relatively strong; and in the case of one and the same individual, we say that it is easily touched off by such and such a set of conditions but not by such and such another set of conditions. Or, again, we discover that for the species as a whole its liveliness is thus and thus functionally dependent upon such general conditions as age, internal physiological conditions, etc. Wells (56), has also specifically refuted this "faculty" charge.

(5) *Fundamental instinct-habit identity.* This indictment has two converse sub-varieties. It is asserted (a) that fundamentally and ultimately considered, habits reduce methodologically to instincts, and (b) that fundamentally and ultimately considered instincts reduce methodologically to habits.

The objection of the (a) variety has been fathered by Dunlap 22, 23). He asserts that, fundamentally considered, the complete stimulus to any act really includes all the previous presentations of the same physical event. We ought, that is, to integrate stimuli temporally as well as spatially. And, if we do so, then habits, like instincts, are but the direct product of innate nature plus stimulus. There is no difference between the two. The point is cleverly taken but is it sound? If all the preceding repetitions of the environmental conditions be considered as part of the effective stimulus, then a habit-act is, to be sure, identical with an instinct-act. But this, while true, is hardly important. For the very point of interest is that in the one case (habit) we *do* have to include all those preceding occasions, and in the other case (instinct), we do not.

The objections of the (b) variety have been, as Wells (56) has pointed out, implicitly or explicitly drawn upon by most of the instinct "deniers." He cites Allport (16), Ayres (17), Bernard (20), Kantor (33), and Kuo (34), as all making use of it. The charge

consists in asserting that all "the responses commonly attributed to instincts are really acquired responses, and therefore habits rather than instincts" (**56**, p. 336). The instinct-responses would not appear if the organisms did not grow up and "learn" them. All actual connections must be acquired since they can appear only *after* and not before the organism has acquired at least a modicum of experience of, and acquaintance with, the objects of its environment. This charge is, perhaps, hardly more than a rewording of the "preestablished harmony and innate ideas" indictment. In reviewing the latter we contented ourselves with merely indicating the lack of any *a priori* or biological reason for *not* assuming at least some degree of preëstablished harmony, *i.e.*, a certain number of preestablished environment-response connections. And we suggested that the whole matter could be decided only on a basis of further empirical observations. Wells, however, comes forward with a more thoroughgoing and fundamental refutation. He points out that even if *all* the supposed instinct stimulus response connections were, one and each, completely acquired, still this does not forbid a useful distinction between such responses and those we attribute to habit. For instinct or "hereditary" responses are ones whose acquisition is "*inevitable* from the fact that it is dependent both on determinors transmitted through the germ-plasm and on constant environmental conditions," *i.e.*, environmental conditions which are an invariable part (due to the organism's own fundamental structure) of the environment peculiar to the species. "Whereas habit or 'acquired' responses, on the other hand, are ones whose acquisition is *not thus inevitable*, ones that *may* or *may not* appear in individual development inasmuch as their appearance depends on environmental conditions alone, and on environmental conditions which are not constant" (*i.e.*, invariable in the above sense) (**56**, p. 340).

We may sum up our evaluations of the five indictments. (1) Nonvariability. This indictment is sound and it has discredited a pure reflex *pattern* type of theory. (2) Arbitrary grouping. This charge, if valid, would hit primarily the teleological theories. It seems to be justified when levelled against the excesses of the sociologists and the economists but it can not in itself discredit the possibility of a "truly psychological grouping." (3) Preëstablished harmony and innate ideas. This charge is aimed at all theories, although it sets most difficulties for a complex reflex pattern type of theory. But it does not hold up against the inevitable assumption of evolution. (4) Faculty psychology. This strictly methodoligical indictment

seems to be pointed most directly at the teleological theories but it fails even in its own "universe of discourse," for the assumption of instinct "faculties" is, on pure methodological grounds, not only permissible but actually demanded. It simplfies and facilitates causal (*i.e., functional*) explanations so very much. (5) Fundamental-instinct-habit identity. This charge would attack any of the instinct theories but it falls down in both its subvarieties, (a) and (b). Instinct-behavior *is* fundamentally different from habit-behavior. For, although undoubtedly inheritance plays its part in both and environment plays its part in both, still variation in heredity are primarily responsible for the differences to be observed in the one and variations in environment for the differences to be found in the other.

So much for the attacks. They clearly discredit the reflex-pattern theory but not the simple reflex nor the teleological theories.

We may now summarize the reformulations of doctrines which they have evoked. They may be grouped as follows:

(1) The complete denial of instincts.

(2) The retention of instincts in the form of simple reflexes (which in the case of man, get immediately covered up by habit).

(3) Reassertions of the teleological theory. We will consider them in order.

(1) The principal complete deniers of instinct seem to be Kuo (**34**, **35**), Faris (**25**), and Ayres (**17**). Of these, however, Mr. Kuo (**35**) seems to have been the only one who has proffered a really positive attempt to explain, in detail, how the complex forms of behavior can be explained without the assumption of instincts. We shall consider his suggestions with some care. Although he denies instincts, he admits certain "units of reaction" as innate. The important thing for him, however, is that these "units of reaction," as innately given, are completely *random*. He says: "By units of reaction I mean the muscular movements of every part of the body of the new-born infant present at birth or shortly after birth. As illustration we may mention again such activities as sneezing, yawning, hiccoughing, various movements of the head, of facial muscles, eye-muscles, of lips and of tongue, swallowing movements, crying and other vocal movements, the movements of the trunk and of the arms and legs in various ways, the extension and flexion of fingers and toes, grasping, sensitivity to sound, light, temperature, etc., and hundreds of others."

It is out of these units of reaction that all the complicated serial

performances which we have been wont to "miscall" instincts get built up. What, for him, is the mechanism of this building up? Why do the units of reaction get integrated one way rather than another? It is because of certain guiding behavior-sets or "response-postures." A "response-posture" he defines in terms of an "end" or "consummatory-reaction," toward which it guides the organism. It consists merely in a patterned heightening and lowering of response thresholds. And the one important thing about it is that it is *acquired*, otherwise it could not be distinguished from some of the behavior-drive formulations of the teleological definitions of instinct.

As to the manner of acquisition of these "response postures" (he discusses it only as regards hunger and sex), he admits (a) specific "intraorganic stimuli which upset the equilibrium of the organism" but denies (b) that "hunger or thirst can produce in a child an eating or drinking response-posture, if it has not eaten or drunk anything before," or that "the disturbance of equilibrium produced by the secretion of the sex glands can make the organism assume the posture of sexual act, normal or perverted, even if the organism has no sexual knowledge before." Are these latter assumptions correct? Possibly so, in the case of humans—but Craig's observations (**18**) certainly indicate them false in the case of doves. For Craig observed the final consummatory behavior *actually going off* in incipient form *prior* to experience, *e.g.*, incipient nesting reactions of male doves on perches, floor, etc. But we may perhaps leave the final decision as regards this point in humans open for further observation.

There is at any rate, one innate feature that Mr. Kuo does admit even in humans and that is that the randomness of the innate units of reaction "is usually restricted within certain fairly definite and discoverable limits" and that "one stimulus will evoke one group of random movements which may not be the same as those called forth by another stimulus of a totally different nature. The movements caused by a loud noise are not the same as those caused by 'petting.'" See in this connection Watson's observations on the fear, rage and love responses on infants (**55**).

To sum up, it seems to the reviewer that insofar as Mr. Kuo does thus admit specific initiating intraorganic stimuli (as in hunger and sex) and does admit specific limitations to the scope of the resultant "units of reaction," he approaches near to all that a behavioristic teleological theory would demand (see Tolman, **52**).

(2) The proposal to retain instincts but only in the form of

relatively simple reflexes (which in the case of man get immediately covered up by habit) seems to have been fathered most specifically by Kantor (**33**) and Bernard (**20**). They use the term reflex *pattern* but from the point of view of our analysis the term *simple* reflex seems more appropriate. Adult humans, according to these authors, exhibit no pure instincts but merely "instinctive-behavior," *i.e.,* habit complexes built up upon the simple instincts. These writers do not attempt to list the simple instincts; their interest is not in them, but in the acquired character of all actual adult behavior. They somewhat naïvely accept the pure rigid reflex *pattern* theory for the behavior of animals.

It is to be observed that these authors have the same problem of describing the development of complex forms of behavior that Mr. Kuo has, although for them the latter is built up out of simple reflexes whereas for Mr. Kuo it was built up out of purely random acts. But they do not attempt to solve their problem in any detail. We can but suspect that they would be driven into much the same teleological assumptions that Mr. Kuo was.

(3) We pass to a consideration of the teleological theories themselves. They fall into three subvarieties: (a) the biological, (b) the mentalistic, and (c) the behavioristic.

(a) A solely biological type of theory has probably been advanced by no one. But a biological flavor has been given to the other types by the tendency (not uncommon) of resorting to biological categories in helping to make up one's "lists" of instincts or of "fundamental behaviors." (It was no doubt this tendency which misled Münsterberg (**41**) into attacking the whole concept of instinct on the ground that it belonged to biology rather than to psychology.) The biological concepts of "self-preservation" and of "race preservation" are constantly cropping up as candidates for the fundamental list of instincts: as, for example in Paton (**42**, p. 244), Holmes (**28**), Warren (**53**). But surely these are neither reflexes nor ends which the individual, as such exhibits. They do not, in other words, fit into any definition of instinct. For they are in no sense properties of individual behavior as such. If the biological variety were the only variety, then indeed the teleological conception of instincts would be in sore straits.

(b) We turn to the mentalistic variety of the teleological theories. McDougall (**38**, **39**) and Drever (**21**) are the chief exponents of this variety. McDougall, as is well known, defines instincts in terms of characteristic accompanying emotions. Drever defines them as

the "life impulse, becoming conscious as determinate conscious impulse" (**21**, p. 88). When we ask how the impulse is determinate, he says that "the conscious impulse becomes a particular conscious impulse with regard to a perceived object or situation" (**21**, p. 88).

The reviewer's criticism of these definitions would be that both, in order to arrive at their specific lists of instincts, have to resort to behavioristic concepts to help themselves out.

Consider, for example, McDougall's definition in terms of emotions. Pugnacity, he defines, as the accompaniment of anger. According to his explicit doctrine, then, anger is the thing which is known and obvious, while pugnacity the thing which has to be inferred therefrom. But implicitly and in actuality his procedure, it seems to the reviewer, has to be just the reverse. For pugnacity, not anger, is the thing actually observed. How are we certain of anger in another individual? Only by virtue of the pugnacity of his behavior? If the other individual did not *behave* in a pugnacious way, we could never be certain that he was angry. Even, if he introspected and told us he was angry, we would not believe him unless his remarks were sooner or later verified by actual pugnacious behavior. In short, it must be the behavior which defines the emotion, rather than the emotion which defines the behavior. This is particularly obvious if we extend the definition of specific instincts (as McDougall does) to the lower animals.

Turning to Drever, he also, it seems, surreptitiously resorts to behavioristic aid in identifying his specific instincts. For how can one "conscious impulse" be distinguished from another save in terms of the respective sort of acts or of environmental (*i.e.*, stimulus) situations toward which it impels. And, indeed, his list of instincts is much the same as anybody else's and includes such items as "hunger, thirst, sleep, sex, experimentation, hunting, curiosity, self-display, self-abasement, parental" and the like (**21**, p. 169). What the difference may be as "conscious impulse" between, say, "thirst" and "curiosity" save one between either the types of behavior or the types of stimulus-object desired, we leave the reader to decide.

c) ~~(3)~~ We turn now, then, to the confessedly *behavioristic* of the teleological theories. Two preliminary remarks may be made.

(One.) It is to be noted that one of the fundamental arguments (or perhaps rationalizations) at the bottom of the mentalists' position seems to be their ineradicable dogma that purposes or ends are sacrosanct and thereby *von Grund aus* incapable of merely naturalistic

treatment. In reply to such a dogma, one may merely point again to the just preceding demonstration that even the mentalists themselves never escape a really non-naturalistic, nonbehavioristic statement of their ends and purposes. And one may also point to the fact that certain writers seem to have specifically proved not only the possibility but the necessity of defining purposes in *primarily* behavioristic terms. See a very remarkable series of articles by Perry (**45**, **46**). See also Tolman (**51**) and Hocking (**32**).

(Two.) It is to be noted that behavioristic definitions are not simply *physiological* definitions. This is a point upon which there tends to be misunderstanding. (Hocking makes this slip, **32**.) The behaviorists do not define instincts solely, or even primarily, in terms of the underlying neuromuscular, neuroglandular or other neurophysiological processes. Such physiological definitions would no doubt be useful and desirable in themselves. But at present they would have to be largely speculative; and secondly and more importantly, they would belong, in any case, to physiology rather than to psychology proper—even a behavioristic psychology. But by behavioristic definitions one does mean definitions which identify instincts in terms of general features about type of stimulus, type of response, or about both type of stimulus and type of response, irrespective of what the underlying physiology may be.

Four actual behavior definitions seem to have been thus far proposed: by Woodworth, Craig, Dunlap, and Tolman, respectively.

(a) We begin with Woodworth's. In the *Dynamic Psychology* (**59**) and more particularly in the *Psychology* (**60**) he proposes that instincts be defined as innate "reaction tendencies" to specific consummatory responses. That is, he divides all responses into *preparatory* and *consummatory*. Consummatory responses are the ones which terminate trains of activity. Preparatory responses are the ones which are necessary in order to obtain the appropriate stimuli for the consummatory responses. For example, in hunger, the chewing and swallowing movements are the consummatory responses, the various searching and hunting activities, the preparatory responses necessary to achieve the stimuli (food in the mouth) for the consummatory responses. The whole train of activity is set going by a *subliminal* arousal which constitutes the *reaction tendency.* And it is the reaction tendency which acts as *drive* to guide and select the preparatory responses. There may be acquired as well as innate reaction tendencies, but instincts are innate reaction tendencies. It appears that these innate tendencies are characterized by specific

exciting stimuli and by specific preparatory responses (although he nowhere treats of these in any clear detail). He says: "A complete account of an instinct would cover the following points: the stimulus that naturally arouses it, the end-result (consummatory response?) at which it is aimed, the preparatory reactions that occur, external and internal; and also from the introspection side, the conscious impulse, the peculiar emotional states (if any) and the special sort of satisfaction that comes when the end-result is reached" (pp. 137, ff).

We would sum up his position thus: The ends which define instincts are *consummatory responses,* although he also suggests that there *may* be—though this is incidental rather than definitive—characteristic *mental* accompaniments of these consummatory responses and of the drives toward them. Further, the instincts possess characteristic exciting stimuli and characteristic subordinate mechanisms (preparatory responses), although he nowhere clearly explicates these details.

(b) We turn to Craig's theory. It is closely related to Woodworth's. Craig (**19**) as a result of extraordinarily valuable observations on the behavior of doves (**18**, **19**) arrives at a concept of appetites and aversions (**19**, p. 91). "An appetite (or appetence, if this term may be used with purely behavioristic meaning) so far as externally observable, is a state of agitation which continues so long as a certain stimulus which may be called the appeted stimulus, is absent. When the appeted stimulus is at length received, it stimulates a consummatory reaction, after which the appetite behavior ceases and is succeeded by a state of relative rest." "An aversion is a state of agitation which continues so long as a certain stimulus, referred to as the disturbing stimulus, is present; but which ceases, being replaced by a state of relative rest, when that stimulus has ceased to act on the sense organs."

The close similarity of Craig's definition of *appetite* to Woodworth's definition of instinct is obvious. They are both ultimately defined in terms of a final or *consummatory* response. And for both, the organism is in a state of tension until this consummatory response is achieved. And it is this state of tension which calls out and directs the "preparatory responses" (Craig does not use this phrase).

Particularly important for the theory, are certain of Craig's actual observations. (1) He observed actual incipient performances of the consummatory response while the organism was in the state of tension and hunting for the appropriate "appeted" stimulus.

Thus male doves were observed tentatively and incipiently performing the nesting reaction (turning round and round as if patting the nest with breast and tail) on perches, floor, etc., until a suitable place such as a box or actual nest, was found. When this was finally hit upon by chance, the consummatory reaction went off in a perfect orgy, complete and full-blown. (2) In the case of very potent drives, the consummatory response, when the appropriate stimulus was not forthcoming, was observed to vent itself in full-blown fashion upon inadequate stimuli (to which it thereupon became firmly attached). Thus male doves, reared in isolation, came to perform the copulatory act upon the keeper's hand or upon a perch. Such doves, when put with their own kind, found it difficult to transfer the response to the appropriate object.

It appears, however, that Craig's complete theory of instincts includes *aversions* as well as appetites. A complete instinct or, as he prefers to call it, a complete instinct cycle has, he believes, four phases:

"Phase I. Absence of a certain stimulus. Physiological state of appetite for that stimulus. Restlessness, varied movements, effort, search. Incipient consummatory action."

"Phase II. Reception of the appeted stimulus. Consummatory reaction in response to that stimulus. State of satisfaction. No restlessness nor search."

"Phase III. Surfeit of the said stimulus, which has now become a disturbing stimulus. State of *aversion*. Restlessness, trial, effort directed toward getting rid of the stimulus."

"Phase IV. Freedom from said stimulus. Physiological state of rest. Inactivity of the tendencies which were active in Phases I, II, and III."

The distribution of emphasis upon these different phases varies for the different instincts. Fear, for example, is an instinct in which Phases I and II are practically lacking, although not totally, since as he points out, we *do* have cases of individuals hunting and courting danger, *i.e.*, the appetite phase of fear.

We may summarize Craig's doctrine as follows: Instincts contain two main phases: an appetite, to obtain the given stimulus requisite for producing a specific consummatory response; and an aversion, away from this appeted stimulus after satiety has set in. As regards the appetite it would appear further that the consummatory response itself is innate and that the appetite for this response is innately attached to some specific intraorganic condition. But the

recognition of the appropriate external stimulus for the consummatory response would seem to be loosely given. For, on occasion, the consummatory response goes off in response to quite inappropriate stimuli. As regards the aversion, it would appear that this is innately attached to the physiological condition of satiety plus the particular stimulus that innately evokes that satiety. Finally, nothing very specific is said about the nature of the subordinate responses, *i.e.*, "the varied movements" and "search," the "restlessness" and "trial" whereby the appetities and aversions eventually achieve their ends.

(c) As the third case of a teleological and behavior definition, we cited Dunlap (24) although he himself might be somewhat surprised at finding himself so classed. For overtly his doctrine seems to be more one of denying than of defending instincts. And yet alongside of this denial, or at the most a retention of instincts only in the form of simple reflexes, he presents a doctrine of "fundamental desires" (24). Now these "fundamental desires" are, to the reviewer' mind, identical with the teleologist's concept of instincts. Dunlap lists them, tentatively, as follows:

1. Desire of aliment.
2. Desire of excretion.
3. Desire of rest (food—drink).
4. Desire of activity (to be rid of disturbing things).
5. Desire of shelter (protection from disagreeable factors in the environment).
6. Desire of conformity (doing as others do or as a leader does).
7. Desire of preëminence (leadership).
8. Desire of progeny (parental desire).
9. Desire of sex gratification (amatory desire).

Ultimately, however, he says they must be identified in a more direct way, and in a footnote he says: "We might speculatively assign the desires to the various parts of the body as follows: (1 and 2) Alimentary canal and urinary system, associated with hunger, thirst, fullness, emptiness. (3 and 4) Striped muscles; strain, relaxation, fatigue, etc. (5) Skin, mucous membranes, relaxation, fatigue, etc. (6 and 7) Circulatory and respiratory systems. (8 and 9) Sexual organs."

The reviewer is not altogether clear as to what Dunlap himself here intends but will assume that the first way of classifying these desires names them in terms of certain typical external *end*-behaviors

or *end*-situations, while this second way of classifying names them, rather, in terms of typical intraorganic stimuli supposed to evoke them.

The "desires" would thus have two definitive characteristics, *i.e.*, they would be defined (*a*) as responses to certain specific types of intraorganic stimulus-situation, and (*b*) as drives toward certain specific types of external *end*-behaviors or *end*-situations. Finally, (*c*) they are further characterized by the fact that it is they which call out and direct the detailed responses.

(*d*) As the last example of a teleological and behavioristic definition, we cited Tolman's (**52**). He attempts to define an instinct as a determining adjustment evoked by certain specific stimuli (external or internal) and aimed towards other specific "appeted" stimuli (to use Craig's phrase), external or internal, which produce *neutralizing* adjustments. The specific stimuli for arousing the initiatory and neutralizing adjustments he conceives to be in the first instance innate. But he supposes that with experience new stimuli may become attached to each. For example, originally it may be only loud noises which frighten one, and only moderate silence which reassures one, but with experience many other things, beside noises, may come to frighten and many others, beside silence, to reassure (see Hunter's interesting discussions, **12**, **29**, **30**, showing that new stimuli may be acquired even before the full-blown appearance of the instinct).

Tolman's second point is that these initiating and neutralizing "determining adjustments" are to be recognized and defined primarily by the type of *external* behavior "organism-object" rearrangement they bring about. Thus "flight" means always a "getting away from"; hunger, always a "searching and eating of"; sex, always a "searching and excitement of the sexual organs from"; curiosity, always a "searching and manipulating of"; and so on. In other words, the determining adjustments (or instincts) are recognized by the teleological patterns of the final goals which they achieve. The subordinate movements and objects involved may be all *acquired,* but the general pattern of the goal is innate and constant.

The present reviewer can hardly hope to conceal his own bias in favor of these teleological and behavior theories although he is quite willing to recognize that no one of them is at present adequate. It may, however, be helpful somewhat carefully to compare and evaluate them as they now stand. We note the following points:

(1) All agree in defining instincts in terms of innate drives or sets ("reaction-tendencies," Woodworth; "appetences and aversions," Craig; "fundamental desires," Dunlap; "determining-adjustments," Tolman). (2) Woodworth and Craig (in the appetencies) agree in defining the goals of these drives as *consummatory responses,* but Craig (in the aversions) defines the goal of the drive simply as the getting away from a particular stimulus; that Dunlap defines the goal sometimes as a particular general type of end-*behavior* (**2, 3, 4, 6, 7, 9**) and sometimes as a particular type of general end-*situation* (**5, 8**); and, finally, that Tolman defines the goal as a particular type of final rearrangement of organism and external object. (3) The initiating stimulus for the drive, according to Dunlap and Craig (in the appetencies) is always "intraorganic," according to Craig (in the aversions), according to Woodworth, and according to Tolman, it may be either intraorganic or external. (4) All four authors allow for what may be called "*secondary responses*" in the service of the drives (*i.e.,* "preparatory responses," Woodworth; "restlessness, varied efforts, trial, search," Craig; "habits and instincts" [the latter in the sense of simple reflexes], Dunlap; and "subordinate responses," Tolman). (5) As to innateness: for all four, the nature of the fundamental underlying drives is innate and permanent. And for Craig and for Dunlap, these drives are innately and permanently attached to specific intraorganic conditions (stimuli). For Woodworth and Tolman they are in the first instance attached to certain specific stimuli; introörganic as in hunger and sex, external as in flight and pugnacity; but through experience they may become attached to quite other stimuli. (6) The "secondary responses" (which according to all four are guided and determined by the instinct-drives) are held to be capable of adaptive adjustment to external conditions. That is, they are only partially, if at all, *innately* and *fixedly* attached to specific external stimuli.

In conclusion, we may sum up a couple of further points of interest: First, attention may be drawn to Moore's observations (**40**) upon the results of testicular and ovarian transplantation. He observed all the details of the male copulatory act occurring in a spayed female with grafted testicular tissue and all the details of the female nursing act occurring in a castrated male with transplanted ovarian tissue. Such facts certainly seem to demand some sort of instinct-hypothesis. The teleological hypothesis, with its allowal of more or less innate reflex patterns as required, would seem to satisfy this demand as well as any other.

Secondly, attention may be drawn to a certain very significant recent movement appearing in the literature, viz., the tendency to describe man's social behavior as immediately due to "over" instinctive ends, *i.e.,* ends which are not innate or biological in any immediate sense, but dependent rather upon the acquisitions arising out of social institutions. It is indeed this recognition of the social institutional character of most of man's fully developed behavior that lies at the bottom of much of the anti-instinct movement we have been reviewing, Ayres (**17**), Faris (**25**). But as Link (**36**), Wieman (**58**), and indeed Bernard (**20**) and Kantor (**33**) have indicated, this proof that man's social behavior is immediately describable in terms of "over"-individual values, does not mean but that there are also innate individual (*i.e.,* "instinct") values. And, indeed, in the last analysis the "over"-individual values must themselves be explained in terms of the "individual" instinct-values (as well as in terms of features of social institutions).

Finally it appears, *prima facie,* as though the teleological definition of instinct would fit in more successfully with this final elucidation of the "over"-individual values than would any conception which defined instincts in terms of reflex mechanisms (simple or complex).

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SPECIAL REVIEWS

Lipmann, O. Bibliographie zur psychologischen Berufsberatung, Berufseignungsforschung und Berufskunde. *Schriften zur Psychologie der Berufseignung und des Wirtschaftslebens,* 1922, 20, 1–49.

This is simply a careful listing of publications up to date in the field of vocational psychology. Not only is the field of German publications well covered, but also a wide range of British, American, French, Italian and Russian articles are listed. The scope of the German material is most extensive and the list most complete, there being 532 German titles in contrast with 128 English and 92 French.

The German titles show a more strict adherence to vocational work in the school and in industry, while in the United States more seems to have been done in the actual making and standardizing of mental and trade tests. France, as judged from this bibliography, seems to have done little in the field of vocational guidance when compared with the vast amount that has been accomplished in Germany and in the United States, or to a somewhat lesser extent in England.

This bibliography is far more complete than that of Muscio, "Vocational Guidance—A Review of the Literature." It is Muscio's review of special investigations in the selection of industrial workers by means of mental and trade tests, that makes his work peculiarly valuable.

Lipmann omits four titles covered by Muscio's earlier summary.

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Rubin, Edgar. *Visuell wahrgenommene Figuren. Studien in Psychologischer Analyse.* I. Teil. Copenhagen: Gyldendalske Boghandel, 1921. Pp. xii+244.

This interesting study, which for the most part is confined to a description of phenomena without theoretical presuppositions or

inferences, has nevertheless an important bearing upon systematic psychology. Begun, as the author tells us, in 1912 in the laboratory of Professor G. E. Müller at Göttingen, its publication was delayed by the war until 1915 when the Danish edition appeared, of which a German translation is now before us. The first part of the monograph deals with the characteristics of "figure" and "ground" in visual perception. With the aid of a projection lantern a series of nonsense figures, bright surfaces on a dark ground, were thrown upon a screen before the observer. After a little practice it was found easy to perceive these, either as bright figures on a black ground—"positive" apprehension—or, taking the black outlines as a margin, to regard the lighted center as a ground upon which a black figure has been placed—"negative" apprehension.

In the first experiments the observer was instructed to assume one of these two attitudes and to perceive all the figures in a given series either as "positive" or "negative" according to the instructions given. After a period of time had elapsed a series including both new and old exposures was given, the observer being now instructed to remain passive during the exposure, and to report immediately thereafter whether the figure was "positive," "negative," or neither. The results of these experiments showed that from 59 to 84 per cent of the repeated figures were apprehended as they had been seen under the instruction of the first series, from which the author concludes that there is a "figural after-effect" (*figurale Nachwirkung*). Further experiments of a similar nature in which the test of the second series was governed by the same instructions as the first, indicated recognition in some 50 per cent of the cases where the instruction of the two series was identical and only 9 to 16 per cent in the cases where the instruction had been reversed; thus revealing a fundamental difference between *figure* and *ground* even when the objective conditions are the same. The difference begins with the contour of the figure and works inward, hence the contour is of greater importance to the figure than it is to the ground, lending as it does a "thing-character" to the figure, whereas the ground has more of a "substance" character. Further observations indicate that shadows are more pronounced when cast upon a figure than when they fall upon its ground; also, that the "surface-color" of the figure is more affected by colored light than is the color of the figure's ground. The last observation leads Rubin to conclude that the color of a figure is always a "surface-color," following Katz's terminology,

while *Oberflächenfarben* or "film colors" are a characteristic of the ground.[1]

In relation to its ground a figure is also more insistent (*eindringlich*), more dominant—and everything that belongs to a figure, its detailed structure, etc., is better remembered than anything of the ground. With a reversible cross whose sectors were alternately red and blue, although the red color was more insistent than the blue, this no longer held true when the blue sectors formed the figure of the cross. The contour which separates two fields normally affects both, but more the field that is figure than the one that is ground; when it affects neither we have what is sometimes called "mental blindness."

A number of conditions may favor the selection of one field rather than another as figure. Among these the first is a tendency to select as figure the smaller and enclosed field. Where there is no enclosure, one selects the lower field in preference to the upper. All repetitions tend to be perceived in the same way as before, and fields which suggest an up and a down or a right and a left are likewise favored over fields in which these fundamental directions are more ambiguous. Finally, colors are preferential factors: red before blue, blue before green, and red also before green. These conditions are not necessarily of original nature but may have become preferential through the uniformity of practical exigencies.

The distinction of figure and ground has a close connection with attention, but whereas attention is a vague and uncertain concept which psychologists are wont to use in helping themselves out of difficulties, we have here a concrete phenomenal distinction that can be readily described. Hence to call this phenomenon a case of attention adds nothing at all to our knowledge of its conditions.

In the second part of his book Rubin studies the surface-figure and its relation to contour and line, pointing out that the contour has neither width nor color. It is the figure which is areal while the contour which encloses the area has length but no breadth. What the psychical construction of a contour or outline is, we do not yet know, and it may be *sui generis*. Neither do we know how far it obtains in other domains of sensitivity apart from vision. A form pressed against the cheek seems to lack contour, but so do visual figures when seen after dark- or light-adaptation of the eyes. In the

[1] This conclusion seems to be in agreement with Martin's experimental work upon this subject. She finds no intermediates between "film" and "surface" and localization of the color appears to be the determining factor for surface-perception or figure. *Cf., Amer. J. of Psychol.*, 1922, **33**, 451–480.

latter cases we can, however, observe something similar, like a border or margin (*Rand*) to the figure.

As regards the distinction of contour and surface-figure, the surface is always one, while the contour though a unity may be membered, as the contour of a square, for instance, has four sides. Here follows an interesting study of the distinction between the contour which determines an angle in the geometrical sense, and the quality of *pointedness* which belongs to the surface enclosed within the angle. For persons geometrically trained the two are interchangeable terms, but not so for the untrained, whose judgments in determining a medium degree of pointedness between blunt and acute angles show a wide divergence from the geometrical medium of 90 degrees. A similar test in which a comparison is made between the pointedness of angles and tongues with curved sides showed that with geometrically trained observers no geometrical uniformity was discoverable in their results. For similar reasons breadth is described as phenomenally quite different from the distance relation between opposite points on the contour of a figure. The independence of contour and surface is further indicated by experiments in the construction of a wavy band, one contour being given, with reference to which the breadth is to be made equal throughout. When the contours of such a band of phenomenally equal breadth were afterwards compared it was found that the curves were by no means congruent. Contours which are curved can also be changed into contours of straight lines without altering the figure which they enclose, as can be easily shown by different types of line-drawings which reproduce the same figure.

Eye-movements are not necessary in following a figure's contour, for the contour can be as readily followed upon an after-image where the eye must remain at rest if the figure is to be at rest.

The writer proceeds next to a study of lines which, if made narrow enough so that one can no longer distinguish one side of the line from the other, are phenomenally without breadth. As such, they may function either as figures or as contours but never as a ground. When compared with a contour a line-figure is more insistent for like the surface-figure it can be apprehended in a very brief exposure, whereas a contour cannot. Some experiments on drawings are here described in which the observers were instructed to reproduce the outlines of nonsense figures such as those employed in the first series of experiments. Among other things it was found that more accurate drawings were made from memory when the

observers were instructed to regard the figure to be reproduced than when they were told to attend especially to its contour. From this the pedagogical conclusion is reached that a method of drawing which emphasizes the mass of the figure is preferable to the one more usually employed in which the outline is emphasized.

In the concluding sections an attempt is made to correlate the phenomenon of unextended visual points with the arousal of a single cone on the retina. The volume closes with a discussion of the views of Locke, Berkeley and Hume on *minimum visibile* which includes the inference that while Hume was successful in demonstrating the existence of nonextended visual objects, he was not justified in identifying them with geometrical elements.

It was stated at the beginning of this review that Rubin's study has important bearings upon systematic psychology. It is perhaps already obvious to the reader that the conception of "figure" and "ground" has a much wider application than to the field of visual perception from which it is derived. Although begun and apparently carried out under the direction of G. E. Müller, who among modern psychologists is perhaps the one most widely known for his influential support of the doctrine of the association of conscious entities, Rubin's data have been eagerly welcomed by the "*Gestalt*" psychologists for whom conscious elements and their association are now obsolete terms in systematic psychology.

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Giese, Fr. *Psychologisches Wörterbuch.* (Teubners kleine Fachwörterbücher, Bd. 7.) Leipzig & Berlin: Teubner, 1921. Pp. 170.

Giese has given us a compact dictionary covering virtually the entire field of psychology and many outlying regions. Its wide range will be seen by the following representative terms: Agraphie, Christian Science, Endogamie, Geotropismus, Homozygot, Konstanzmethode, Labyrinth, Libido, Panpsychismus, Perzeptionsumfang, Physiognomik, Plethysmograph, Tappingtest.

The definitions are brief and to the point. They are generally stated in untechnical language, intelligible to the lay mind. When a word in the definition is used in a technical sense, it is marked with an asterisk, calling attention at once to its special significance. The

special methods and procedures used in psychological research are explained, and where possible the name of the originator is indicated in brackets before the definition. The principal psychological apparatus are described, and in many cases are illustrated by a figure, simply drawn, but giving the essential features.

In addition, there are short biographical references to leading writers, with date of birth (and death) and principal works. At the end of the volume is a brief list of books in each of the chief fields of psychology to assist the general reader, and an extensive list of current psychological magazines; both of these are confined to German publications.

Giese's work should be of considerable value to the psychologists of English-speaking lands, especially in the absence of any up-to-date technical dictionary in our language. Exception might be taken in some cases to the author's failure to notice certain alternative meanings, but this could scarcely be expected in a volume of this size. On the other hand, too much space is devoted to one particular classification of tests, under Eignungsprüfungen (over 3 pp.), and to phonetics, under Lautartikulation (nearly 2 pp.). One may also question the appropriateness and truth of the following statement made with reference to telepathy: "Eine Erklärung der Erscheinung, die wohl nicht mehr bestritten werden kann, ist bis jetzt nicht erfolgt."

As might be expected, the choice of writers for biographical mention shows the local perspective. The data will prove a great help to those seeking information about German psychologists of all ranks, but other lands are thinly represented. Among American writers we find James, Hall, Baldwin, Titchener, and Münsterberg (who died at Harvard, Neuyork!); but we look in vain for Ladd, Cattell, Jastrow, Thorndike, and Watson, whose names would appear in any American compilation.

Outside of general psychological terms, the fields most fully covered are psychophysics, pathology, the psychoanalytic dialect, mental tests, and anthropology. The book is weakest on the side of physiological psychology and animal behavior.

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